November 29, 1950

Dr. Joshua Lederberg
Department of Genetics
University of Wisconsin
Madison 6, Wisconsin

Dear Joshua:

I have given some thought to the questions raised in your letter of the 18th about the source book on microbial genetics and transmit them for your consideration.

From the point of view of a teacher of genetics of microorganisms, I have asked myself: "What kinds of papers would be most useful to students of this field?" There could be several bases for selection. First, one could select those papers in which the germ of an important idea, method, or discovery first appeared. Second, one could select, without reference to priority or origins, the papers which best illustrate ideas, methods or areas of discovery. Third, one could select reviews, discussions, or integrative papers. Fourth, one could select those long and complex papers to which the student may be repeatedly referred and which can scarcely ever be dealt with adequately in lectures.

Ideally, I should like to see enough of all four types of papers included so as to cover the field adequately from these points of view. If any type were excluded, it should doubtless be the third. But then this omission should be supplemented by a bibliography of such papers. The latter could be managed on a page or two and would add greatly to the value of the book. I therefore strongly recommend that such a bibliography be included and would be glad to contribute suggestions as to useful titles for it.

The next group that offers difficulties, mainly because of size of papers, is the fourth. For example, I consider Whitehouse's paper of 1942 in the New Phytologist (41:25) as one of the more important papers on genetics of Neurospora, as one that can be properly dealt with in lectures only at the expense of a disproportionate amount of time, and therefore as one that would be among the most useful for a student to have available. Your limitations of size doubtless require you to omit such papers. That seems to me a great and almost fatal difficulty.

You have confined your list in the main to papers that fall in my first two classes. Actually, it seems to me you have not

been quite consistent about this, though in your letter I get the impression you wished to emphasize my class 2. Apparently the size of paper has made you prefer in some cases the first paper to the one that best illustrates the point.

I therefore wish to raise, as others seem to have done, the question of whether it would be better to try to do a proper job with a more limited field rather than sacrifice standards in order to encompass in an unsatisfactory manner a broader field. By restricting the volume to bacteria you would have an additional 150 pages available and this might be enough to do a good job on that restricted field. Personally, I'd rather see a good job done on bacterial genetics than an unsatisfactory attempt to cover a broader area. Perhaps it could be indicated in the preface that, if this book is successful, similar source books for other areas of the genetics of microorganisms will be forthcoming.

I also like Luria's suggestion about a loose-leaf binder. If that were done, it might also be stated that periodically supplements to the bacterial volume would be forthcoming. A problem in this connection is the reaction of libraries. It would, I am sure, be quite impossible to prevent filching of papers from a library copy of a loose-leaf book; and I suspect that libraries will not wish to purchase a book set up in that way. However, the book is not intended for libraries and this need not be an important consideration.

If you decide to go ahead with a volume of broader scope--which I strongly urge you will not do--then the following suggestions as to deletions and inclusions might be considered.

The bacterial section would, in my opinion, profit by the inclusion of one of Robinow's papers on cytology, by one or more papers of the Stone group, by your paper on the exclusion of alternatives to sexual fusion; and perhaps also one of Witkin's, one of Lanarjet's and one of Bunting's.

As to the Phages, I am not prepared to make recommendations for until now I have left them out of my course, since Luria covered that material. Now, alas, I shall have to do my best to make good our loss.

Your selection of papers on Paramecium would be improved, I think, by deleting part II of my 1945 paper and replacing this by my 1937 paper in PNAS, 23, which (if I may express an opinion about my own papers) was probably my most important contribution. Further, the complete omission of the work on P. bursaria seems to me hard to justify and I would not, even by remote implication, wish to take any responsibility for it. If you think better of this, I'd like to suggest one or more of the following for consideration: Jennings 1938, PNAS, 24:112-120; Jennings 1941, Proc. Amer. Phil. Soc. 85:25-48; Jennings 1942, Genetics 27:193-211; Chen, 1940, PNAS, 26:239-240 or 1940 J. Hered. 31:175-184. Among the early papers on Paramecium genetics, some are models of a type of study

and presentation of results with which modern students should be acquainted. Among these I would particularly list Jennings 1908, J.E.Z. 5:577-632; Jennings 1908, Proc. Amer. Phil. Soc. 47:393-546; Jennings 1911, J.E.Z. 11:1-134; Jennings, 1913, J.E.Z. 14:279-391; Jennings 1916, Genetics 1:407:534;(this one is on Difflugia); Jennings and Hargitt 1910, J. Morph. 21:495-561; Jollos 1921, Arch. f. Protistenkunde :1-222. The preceding 7 papers, with which every student of the genetics of Protozoa should be directly acquainted total 869 pages. You rightly guess that I have my tongue in my cheek at this point. But then the question arises, what good is a source book if it doesn't include the basic source materials? Do not think I am trying to stretch the points overly far. Those papers are really important and they contain much that is merely being rediscovered or better explained now-a-days. Further, I have omitted much of the more recent work that should be included if the book is really to serve its purpose. For example, it would be grossly unjustifiable to omit Kimball's contributions; to mention only one "must" there is his paper on inheritance of mating types in Euplotes, Genetics 27:269-285 (1942), not to mention his other papers on Euplotes and Paramecium.

This brings up two other points. First, no source book of reasonable size can include single papers that run more than 100 pages alone. Yet not all important source papers are brief. A possible way out of this is to select a representative portion of such papers. And this principle of selection might even be applied to briefer papers. I am aware of the obvious objections to such a procedure and also of the great burden of time and responsibility this would place on the editor; but it would greatly increase the number of workers and papers that could be included and I suspect that it could be done so as to reduce to negligible proportions the values lost by such deletions.

My second point is that the scheme proposed by your list of papers implies a serious distortion of values. By devoting half of the book to bacterial genetics, by reducing the representation of Paramecium and Neurospora to three papers each, by omitting papers on Yeast and Chalmydomonas, you unavoidably give the impression that the genetics of bacteria is the main thing, with relatively little on other organisms being equally worthy of inclusion. Nothing you can say in the Introduction will effectively counteract that impression. Consequently, it seems to me that, in fairness and honesty, you must either reduce the section on bacteria to a more reasonable proportion of the whole, with corresponding expansion of other section; or expand the bacterial section to the full book, omitting the others completely. In my opinion, the former would yield a book of relatively little value, while the latter would yield a book of great value.

To turn to the Neurospora section, I'm very pleased that you selected the Beadle and Coonradt paper, for that was an outstanding contribution. I also favor including the Beadle and Tatum paper and the Lindegren paper. For minimum requirements, it would seem to

me that the following should also be included: McClintock 1945, Am. J. Bot. 32:671; Srb and Horowitz 1944, J. Biol. Chem. 154:129; Ryan and Lederberg 1946, PNAS 32:163-173; Wagner and Guirard 1948, PNAS 34:398; and Horowitz 1945, PNAS 31:153. I have already discussed Whitehouse's paper. I feel also that it is highly desirable to include a paper by Dodge, perhaps his 1929 or 1930 paper in Mycologia (21:222 or 22:9). This does not do justice to the work of Tatum, Bonner, Mitchell, or Emerson. On the other hand, limiting representation of the Neurospora work, as is done in your proposed list, would seem to me completely indefensible.

For work on other Fungi, I would prefer other papers to the one of Keitt and Langford. For example, I'd like to include: Blakeslee's 1904 paper on sex in Mucors and Newton's 1926 paper on the analysis of spore arrangement in Coprinus, for these were of great historical importance and served as models for much of what followed. Among the more recent papers, I'd select Hansen and Snyder 1946, PNAS 32:272 on Hypomyces (important among other things in relation to the work of Moewus); Fries 1948, Hereditas 34:338 (on selection of mutants in Ophiostoma by starvation) and, if possible, his 1946 paper in Svensk Bot. Tidsk. 40:127; and Pontecorvo and Gemmell 1944, Nature 154:514 and 532, especially the latter. It would be still better if Pontecorvo's work could be even more fully presented.

The Yeast problem is a difficult one, in view of what you say about the copyrights on the papers of Winge and his collaborators. I would, however, include the following papers by others: Lindegren and Lindegren 1948, (Proc. 8th Int. Congr. Gen.) (interpretation of linkage and crossing over); Lindegren, Spiegelman and Lindegren 1944, PNAS 30:346; Lindegren and Lindegren 1946, CSH 11:115; and Spiegelman 1946, CSH 11:256,--all on the plasmagene stuff. Here more than in most cases, the idea of selecting <u>parts</u> of papers for inclusion would pay off. Could you get permission to publish <u>part</u> of Winge and Laustsen 1939 (two papers), 1940, and Winge and Roberts 1948? By judicious selection of parts of the nine papers mentioned, one could present the meat of the Yeast situation without taking too much space. Of the Ephrussi papers, two should suffice: the second and fifth in the Annales de L'Inst. Past. (Ephrussi, Hottinguer and Tavlitski, vol. 76; and Slonimski and Ephrussi, vol. 77). If it comes out in time, the new paper on the gene mutant would make a fine third part to the trinity. Again, it would be advantageous to select only parts of these papers.

Finally, as to Chlamydomonas and Protosiphon, I'd recommend the following: Arch. Prot. 1935, 86:1-57; Biol. Zentralbl 1935, 55:293-309; ibid, 58:516-536 (1938); Zeitschr f. Naturforsch 3b: 279-290; Portugaliae Acta Biol. Ser. A, 1949, 161-199. The three papers of 1940 (Biol. Zbl. 60:143-166, 597-626, and Z. i. Abst. v. Ver. 78:418-522, especially the latter) are of particular interest since they anticipate the whole subsequent development of biochemical genetics and manifest a remarkable grasp of its potentialities and significance in the broadest way.

Now where does all this leave us? Obviously, I have not helped you solve your problems or answer the questions you raised. But I would not consider a source book which included only two or three papers on any organism to be sufficiently serviceable to require students to purchase it. I could only recommend it as saving a few trips to the library. Also, if the German papers were translated, this might be an inducement.

I can only urge that you consider the various alternatives: (1) Confine the book to bacteria. (2) Expand it greatly so as to represent other organisms adequately. (3) Reduce the space devoted to bacteria to bring that section to something like its proper proportion of the whole. (4) Decide upon selecting the more useful and important parts of the papers, instead of including each paper in full.

Your problems as editor are terrific and I wish I could see an easier way out than confining the book to bacteria, but I don't. If you decide to include my papers, let me know and I'll lend you my own last copies.

With best wishes,

Cordially,

TMS:gg

T. M. Sonneborn